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On Trade and Culture Process in Prehistory

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On Trade and Culture Process in Prehistory

by LEO S. KLEJN

Leningrad, U.S.S.R. 14 VII 69

In its anathemas against migrationism and diffusionism, Renfrew's article (CA 10:151-69) seems to me to bear a strong resemblance to Soviet archaeologists' articles of the '30's or '40's and early '50's (Bogayevskiy 1930; Meščaninov 1931; Kričevskiy 1933; Artamonov 1947; Mongait 1952; Arcikhovskiy 1953: 63-64), and in its criticism of evolutionism, to articles in the Soviet literature of the late '40's and early '50's (Levin 1947; Kuznecov and Dmitravkov 1948; Potekhin 1949; Bernštam 1949; Gorbačeva 1952). Of course, there are some differences, not only in tone, but also in some more substantial respects. All the same, the similarity is striking. The influence of the Marxist approach to the materials of archaeology was introduced in Britain by Childe and, to some extent, Grahame Clark. Both adopted the Marxist approach consciously; their successors seem to adopt it without realizing it. The call to study first of all the inherent stimuli of social development has long resounded in our scholarship, and it is still heeded by archaeologists of Marxist orientation. But we have gone far beyond the simplifications and exaggerations of the '30's and '40's (cf. Kričevskiy 1946; Br'usov 1957a, b, c; Amalrik and Mongait 1960, 1966: 262-72; Grigoryev 1964). How funny it is to meet them again in the British archaeology of the late '60's! Must we wait for a new Childe to bring to British archaeology a more up-to-date Marxist understanding of these problems?

Renfrew's preference for "the function, rather than just the form" (pp. 155-56) may be somehow related to the recent English translation of Semenov's valuable book, in which exactly such a view is propagated (1964:6, cf. 1940); but this very opinion of Semenov's has met with objections from other Soviet and foreign archaeologists (cf. Valoch 1957). Indeed, how are we to get to know function if not through form?

Renfrew supposes that to explain innovations by way of inherent laws of development is to deny migration and diffusion. But inherent factors of development act in all human societies, whether stationary or moving, and cause different innovations, both local and introduced from outside. Schliemann's *depos amphikypellon* was of course a fremd, borrowed form in prehistoric Greece, but it was a local one in Anatolia. The crux is to distinguish correctly these kinds of innovations and then not to confine ourselves to identifying the source of the innovation, but to get to its cause. This

means extending our study to encompass such a broad range of materials that the innovation, whatever its source, can be seen as an internal event.

Renfrew's historiographic review has suffered foreshortening by restricting itself to the examination of the phenomena of invasion, diffusion, and evolution. We may consider these phenomena as possible answers to a question as to the *causes of innovations*. There are three of these, and they appear as *invasion, borrowing, and local (autochthonous) development*. If we approach the problem in another way, by asking for the possible *causes of cultural similarity*, then the answers must include *migration, influence, and convergent development (convergence)*, and also *chance coincidence, genetic relation, and penetration of imports*. Different *schools ("theories")* will place different degrees of emphasis on each of these answers, and the succession of the positions will be other than in Renfrew's review. These ideas refer to (and are usually also named after) phenomena that have been favourite or even exclusive means for explaining the interrelations of archaeological evidence (especially innovations and similarities) in the attempt to recover the course and nature of cultural and historical process.

Evolutionism as a trend in the humanities did not follow *migrationism* and *diffusionism*, but was followed by them. Its main features were progress, evolution (as a gradual development), and the unity of laws of mankind's development (it is better to call this *parallel* development than *multilinear* evolution, for the latter term is used in quite opposite meaning, i.e., to designate dissimilar development [cf. Steward's comment, CA 10:164-65, and Daniel 1968]). Migrationism and diffusionism were characterized by the attempt to explain everything by means of migration and diffusion respectively (diffusionism acknowledged migrations but considered them less important than influences). Both trends were founded roughly simultaneously, and another arose at the same time—*cyclism*, with its attention to cyclical repetitions in history and cultural development and its tendency to modernize ancient phenomena.

A concept known as the "*theory of stageness*" ("*teoriya stadijalnosti*") arose in Soviet archaeology in the late '20's. The idea of "stageness" itself (which meant not simply phasic development or development by stages, but development by qualitative leaps) was only one of its positions; among the others were economic (or, more exactly, productional) determinism; the unity of laws of mankind's development (where the unity

was considered just as in the evolutionist mind, i.e., as identity, sameness); and general autochthonicity (hyperautochthonism). As a system of views, this concept lasted till the middle of the century. Although some of the positions—such as primacy of production, the unity of developmental laws, development by leaps (all this, of course, in a broad sense)—have stood firm in the system of Marxist views of history (and no wonder, for they are the general principles of dialectical and historical materialism), the "theory of stageness" as such has been abandoned, and no new theory for archaeology based on these principles has been suggested to replace it. Empirical research has prevailed in the Soviet archaeology of the '50's and '60's.

At the same time, or a little earlier, the main pre-war schools in the West were wrecked, as has recently been noted by Hawkes (1968:261). Just as in recent years some English scholars have criticised contemporary Soviet archaeology by ascribing to it the views of former decades, so some Soviet archaeologists still castigate their old enemies, migrationism and diffusionism, and are oblivious of being in combat with shades of the past. Renfrew does the same. Weinberg is right in considering this as flogging a dead dog.

True, remnants of migrationism and diffusionism are still to be discerned in concrete studies, but surely they must no longer be regarded as the main danger. I do not see any evolutionism in the views of the American archaeologists Steward, Willey, and Phillips; they are, I admit, trying to discover common stages of development for the whole of mankind, but the urge to discover such stages existed before evolutionism (in the "Three Ages" system), simultaneously with it (in the Marxist system of the social and economic structures—the primitive communal one, the slaveowning one, the feudal one, and so on), and after it (in the "theory of stageness" and in the American archaeological sequence of Lithic, Archaic, Formative, Classic, and so on). In some systems this unity of stages is limited to a certain sphere and even in this sphere is combined with a statement of different ways of development.

Only one of the great schools of the recent past has retained its importance: the *ecologist* school, with its geographic determinism. The modern version of this school (in the spirit of Grahame Clark) differs from the old version (Crawford-Fox's) in that the coercion of the environment is seen as acting mainly through the economy of the society. Thus, though, as before, the motive force of development has been placed outside the social sphere—in the environment—

within the boundaries of the social sphere it is the economy that is considered as the first perceiving and refracting medium, and therefore as the foundation of social development. It is in its attention to the economy that the ecologist school approaches Marxism.

The other features of this school are preference for environmental sources of innovation, an interest in natural remains, a passion for "technical" (physical, chemical, biological, geological, mathematical) methods, a tendency to prefer local laws to general ones, etc. Renfrew, who in all his research aspirations and methods belongs to this school, moves a step closer to Marxism than Clark by placing the stimulus of development in the social sphere, in the economy—though not in production, as Marxists do, but in trade. While he recognizes the interaction and near-equality of all the factors in social and cultural life (a touch of pluralism—of the so-called theory of factors—that one can observe also in earlier works of the ecologist school, e.g., Clark's), he considers trade as the main factor (*primus inter pares?*), and in his "cabalistic diagram" (Fig. 2)—which in unique manner combines the Shield of David with the five-pointed star—it is trade that occupies the focal point.

I will not dwell at length on exposing Renfrew's conception of prehistoric trade as too diffuse and broad: it fails to take into account, for example, the differences between commerce, exchange, and any peaceful movement of materials and goods (and how can you distinguish such "peaceful" imports from tribute or loot?); the fact that exchange could be intracommunal, intratribal, intertribal, international (interethnic), or overseas; the fact that it could be direct or transitional; and the fact that both the functional borders of these forms and the chronological ones are different. Another thing is more important in this context: that trade/exchange in fact cannot have been the initial cause, the original moving force, the main stimulus in the mechanism of the social development.

Renfrew hopes to demonstrate his idea with the example of the role of trade in the transition to civilization or urbanization. All these terms—not just trade, mentioned above—need more precise definition.

The literal meaning of the term "civilization" is "transition to the status and relations of citizenship or civility," i.e., the rise of the state. Civilization, according to Morgan, is a society that possesses a written language. In Marxist historiography the term is used for the class society (as distinct from the preclass one). Every term is conventional, and it is a matter of convenience, efficiency, and

general agreement to what concept it is attached; but the concept itself must be defined clearly, logically, and in accordance with the real boundaries in the materials. Each of the above concepts pretending to the term "civilization" fulfils these criteria. Anyhow, each has a right to possess its own term.

Kluckhohn proposed (and Renfrew accepts as "now-conventional," p. 158) a more complicated interpretation: one may call "civilization" a society which possesses at least two of the three following features: writing, ceremonial or religious centres, and settlements with a population greater than 5,000. The logic of this definition escapes me. I can see that one might distinguish societies possessing a certain combination of these (or similar) features and use a special term for them, maybe even the term "civilization." But obviously, this would be reasonable only if this combination were of great importance and uniformly affected the functioning of such societies—and in that case it would be precisely this uniform effect that must be put into words and made the basis for the definition.

It seems, however, that nobody has proved that the combination of these three features has such an effect. Still less probable is it that a combination of two of the three will have such an effect. Kluckhohn wanted a definition that would be empirical, all-embracing, universal, inclusive of all the existing uses, and therefore most objective, but as a result he produced the most arbitrary of all definitions.

Renfrew advances one more feature of developed society: the presence of towns. One might, of course, make this feature a criterion of "civilization"—but there are too many concepts for one term as it is. Thank God, Renfrew has another term—"urbanization." But what is *urbs*? What are town and city? The Russian language does not have this distinction, (paralleled by the German *Burg* and *Stadt*); it has only one term: *gorod*.

The *urbs* (*gorod*, town/city) is distinguished from the village or from the prehistoric primitive settlement in a number of ways: by stone-built houses, fortifications, great size, concentration of nonagriculturalist population (merchants, craftsmen, warriors, priests, and others), functions of administrative, religious, and/or trading (market) centre, and so on. But these features do not appear all together and, in addition, each one of them appears in varying degrees. It is evident that here, too, one term and even two terms (town-city, *Burg-Stadt*, protourbanization-urbanization) are not sufficient for serious research.

Above all, in determining degrees of development, of complexity and

modernization of society, one must not outline a priori artificial or conventional criteria and then fit the real picture to them, but first study this real life and then glean terms for the stages it presents. In this process, it will become clear that among these stages are some that appear only after trade/exchange has developed. Some of these are indeed (at least partly) its consequences: intensification of contacts, travel, widening horizons, enlarging the range of common culture. But it is not just these consequences that determine the history of society. There are some phenomena that merely have causes in common with trade/exchange or are parts of it (or of its forms—the appearance of merchants, the rise and growth of the market functions of towns)—so their confrontation with trade is tautologous. Most important are the phenomena that are in opposite interrelation with trade/exchange, i.e. those that call forth its rise, persistence, and development. In order for the need to move certain goods to appear, these goods must be limited in quantity and unequally distributed over the world. But further, in order for the idea of reciprocity to appear, the receiver must have something equivalent in value to the "gift" and the giver a use for it. In the prehistoric economy, however, such circumstances were rare. As long as the available means of subsistence were barely sufficient to support life, it was impossible to appropriate any of them for trade/exchange. The possession of raw materials that neighbours lacked might have permitted trade; but materials that were important for life were most likely abundant in the neighbours' area too, or that area would have been simply unable to support life and uninhabited. And if, on the other hand, they were not essential, the neighbours would be ready to exchange only if they possessed a rare commodity, too, i.e. if they could offer something equivalent in value. Thus, the likelihood of trade/exchange here was minimal. Therefore, until man had reached a level of productive development sufficient to ensure the accumulation of surplus, trade/exchange could not appear; for the conditions for its appearance were not present below that level.

It is evident that the Neolithic, with its producing economy, is the time when surpluses of goods began to be accumulated—so that there was something to offer in exchange, something to trade in. At the same time, geographic differences were becoming more pronounced, and regional specialization was developing—so that there was something to inquire about, something to need. The surpluses, however, were in commodities (products of stock-breeding and agriculture) that usually do not reach the archaeologist.

He can observe the intensification of trade/exchange in Neolithic materials only in its collateral effects—the increase in traffic in rare natural objects (minerals, shells, and so on). Probably these were drawn into the intensified exchange as equivalent values, sometimes replacing more important commodities.

The introduction of metallurgy gave a new impulse to the development of exchange: only in some regions are there ores; the necessary know-how and experience were readily monopolized (from a ready-made artifact, it would be impossible for prehistoric man to discover the secret of producing it); and unevenness of the development of different regions increased sharply. At the same time, the need for metal artifacts quickly spread far and wide. It has always been true that one may not with impunity lag behind one's neighbours in modernization of armaments for long. And in all times, one can assume, it has been inexcusable to let one's rival present to the woman the rarest and most modern ornaments for exciting envy in her female friends. The greater productivity of the new tools must also have soon engendered a demand for metal, while using them would have given rise to the accumulation of surplus useful to exchange.

Is the comparison with the chicken and the egg proposed by Renfrew apt here? In his reply to his commentators he conceals himself again behind the shield of pluralism. Which came first: the chicken or the egg? Every scientist knows that the answer is "Neither": what came first were the pangolins (if we choose not to look even farther back), and the egg-laying chicken developed out of the pangolins through gross change of environment and natural selection.

Which came first: new demand or new supply? Neither, or rather both. Trade or need of it? The same answer. Innovation or need of it? Here we can give a less ambiguous answer: probably the need, for the need for innovation is in the nature of human beings; people want to live better and try to find means to lighten their work. Thus, for demand and supply and for trade and innovations and the need of them, the only correct final answer is the following: first new possibilities appeared, and these possibilities were raised by new abilities, and these abilities were based on new productive forces, primarily new tools. These last are placed in the upper corner of Renfrew's "cabalistic diagram," and in small print. To support such an underestimation, no cabalism can help.

Renfrew calls his idea a "culture-process model." He agrees with American archaeologists (p. 153) in

regarded rather as aggregates of systems, each of which varies independently. Such systems must be studied in isolation in order to contribute to an understanding of the whole.

Among these systems or rather sub-systems are agriculture, stock-breeding, fishing, metallurgy, exchange or trade, social organization, and so on. Trade is sometimes treated by Renfrew as having the same importance as the other interacting factors (pp. 151, 153, 167), at other times as the main and determining one (pp. 154, 160). The connection among these factors is declared by him to be now free, loose, and weak (p. 153, where he demands that they be studied in isolation), now close and inseparable (p. 167, where he considers it impossible to isolate them). Renfrew oscillates between pluralism and monism. What remains invariable, however, through all these oscillations, and is formulated as a rule and emphasized in the name of his idea, is his refusal to study the archaeological cultures (in Childe's definition) as units.

This position of Renfrew's has much in common with Higham's statement in the same issue of *CA* (p. 149) that

the development of sophisticated absolute-dating techniques has diminished the importance of typological analyses and opened up the possibility of writing prehistory without the encumbrance of meaningless cultural tags.

In Daniel's writings (1950:246) and in Müller-Karpe's (1966:187), we can find similar statements. These are not occasional slips of the tongue or mere words—we are dealing here with a definite trend in contemporary archaeology, a trend which is being incarnated in research methods and principles. It goes without saying that the monographic study of each of the above factors may be of use, but why set off such study against the integrated, comprehensive study of an entire archaeological culture?

It is a bitter irony of fate that Renfrew should have chosen as his prime example of the quantitative study of trade the very case that would unambiguously disprove the basic assumption of his method. By twice demonstrating the steep fall-off of the quantity of obsidian beyond the 300-km. border from the natural sources (Fig. 1, p. 157) and ascertaining in both cases the coincidence of this 300-km. or 400-km. supply zone with an archaeological culture typologically defined (p. 157), Renfrew has shown that it is precisely the boundary of that archaeological culture that determines the sharp change in the conditions and the character of trade. This means that the nature and the position of trade/exchange of one and the same

commodity were different in different cultures, and that besides tracing this commodity and its circulation over the earth, one needs to study the conditions of its circulation in individual cultures. Objects can of course be found the quantitative distribution of which is identical in two or more cultures, but the qualitative features of their circulation in the conditions of different cultures will be different, and their place in these cultures' economies will coincide only in exceptional cases. Moreover, even such cases could be brought to light only by means of comparison of cultures studied separately.

As regards Renfrew's attempt to combine trade/exchange monism with pluralism, it seems to me these principles are as incompatible as the five-pointed star and the Shield of David.

by GARY A. WRIGHT

Cleveland, Ohio, U.S.A. 2 VII 69

A discussion of the type offered by Renfrew has been necessary for many years. Too long have archaeologists fallen back on merely citing "invasion," "migration," "stimulus diffusion," or "trade" as explanations for cultural change. Too often have we had to rely on the competence or honesty of the archaeologist offering the proposition that migration or invasion is the best (and only) explanation for the changes observed by him at his site or in his area. Too seldom have these scholars formulated any hypotheses which could be tested in the field.

Particularly valid are Renfrew's distinctions among different kinds of systems, such as local subsistence, local trade, and long-distance trade (cf. Harding 1967, Wright 1969). Equally important are his attempts to deal with these systems on a quantitative basis, a very difficult undertaking due to the past nature of archaeological sampling (or nonsampling?). Animal bones have usually been discarded as "useless"; flint and obsidian flakes and chipping debris have not been counted but relegated to the back-dirt pile. Thus, valuable qualitative data have been lost.

Two points in Renfrew's paper, only touched upon in the comments, deserve further elaboration by Renfrew. The first of these concerns the lapse rate model used by Renfrew as a convenient descriptive model of Near Eastern obsidian trade.

Independent studies by neutron activation analysis of obsidian samples from the Near East (including samples submitted by Renfrew) have clearly substantiated the trade routes postulated by Renfrew and his colleagues (see, e.g., Wright 1969, Wright and Gordus 1969). Yet a

the insistence that culture or cultures cannot usefully be studied as units, . . . but must be

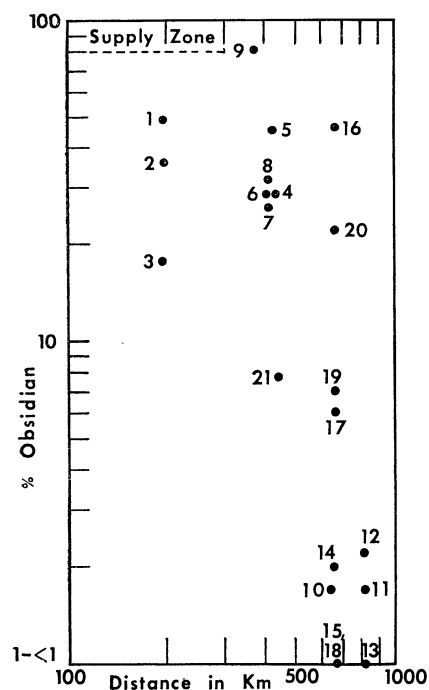


FIG. 1. Distances from Lake Van obsidian sources and percentages of obsidian on archaeological sites dating between 7500 and 5500 B.C.

number of problems seem to arise in regard to the particular use of the descriptive model he has chosen.

1. It appears that, for the period of *ca.* 7500 to 5500 B.C. in the Near East, the weight of obsidian on a site, rather than percentages of industry, is a more relevant factor. During this time period transportation was by human agency rather than by pack animal. Thus, an obsidian core weighing 15 lb. would count proportionally more than a blade weighing 0.1 oz. Percentagewise, there would be no difference between the two samples. Transportation decisions would be weight-dependent, and might also vary according to the form in which the material was transported—blocks or blades—and also according to the ultimate product—microliths or bowls.

Of course, weight is a difficult factor with which to deal because of the way lithic material was handled in older excavations. Chipping debris was ignored, duplicates usually were not registered and not counted, and screening was seldom undertaken. More important, however, is the fact that a weight estimate per size of sample, at say 7000 B.C., cannot be indiscriminately applied to the same raw material at a site dating at 3500 B.C. Neither can a weight constant be indiscriminately applied in different regions, as Renfrew (1969; Renfrew, Dixon, and Cann 1966) found when he used his obsidian weight constant for Saliagos at Jarmo. He has now reduced his weight figure for Jarmo

from 4,000 kg. (4 tons), based on an estimate of 4 kg. per 1,000 pieces (the Saliagos estimate), to 20 g. per thousand (the observed Jarmo estimate) and a total weight of 196 kg.

2. A second question arises when attempting to compare sites which are functionally nonequivalent. For example, in the lapse rate model Renfrew shows in his Fig. 1 (CA 10:157), Sarab, although closer to the supply zone than Tepe Guran, has considerably less obsidian. Why doesn't it fall close to its predicted value of *ca.* 10%, rather than at 1.7%?

Sarab, a herder encampment, was certainly a functionally different type of site from either Tepe Guran or the contemporary ceramic phase occupation at Jarmo. This functional difference is clear from Hole's (1961) analysis of the chipped stone industries of Sarab and Jarmo (Wright 1969).

Might we be able to isolate functionally different types of sites from Renfrew's model? Could we predict that a herder encampment (Sarab), a ceramic specialty village (Yunus Kilns), and a copper-producing site (Tal-i-Iblis), will all plot well off the "best-fitting line" describing the distance-percentage (or weight) transport of a raw material when these kinds of sites are compared to a number of settled, nonspecialty, farming villages?

3. Such predictions would depend upon whether certain variables can be held constant. The generalizations Renfrew has drawn from the lapse rate

model seem to hold strictly true only for the Central Anatolian supply zone, but not for the Lake Van supply zone (Wright 1969). Figure 1 is a replotting of the sites dating between *ca.* 7500 and 5500 B.C. which utilized obsidian from Lake Van sources. Names of sites and figures are given in Table 1. I have made two changes: (a) I have added data not available to Renfrew (e.g., Çayönü), and (b) I have not considered sites as single data points, but have plotted their analytic levels (e.g., Jarmo I and II).

Only one of these sites, Çayönü, lies within the 300-km. supply zone. On the basis of the model, it should show 80% or more obsidian. The counts from bottom to top for the three analytic levels are, 17.6, 35.4, and 48.2% (R. J. Braidwood, personal communication). Tell Shemshara, with 81% obsidian, lies 65 km. outside the hypothesized supply zone. According to the predictive model, no level at Tepe Guran should show over 7.5% obsidian. As a totality, however, the site stands at *ca.* 10%; its separate levels vary from 0 to 46%.

In addition to site function another major variable seems to be the availability of flint. Where the model works best, according to Renfrew's predictions, is Central Anatolia. In Central Anatolia, naturally occurring "obsidian is abundant, but flint is absent" (Mellaart 1967:16). In contrast, flint sources are known in the Lake Van region and throughout the Zagros Mountains (e.g., Murian). Thus, we must consider the problem of "intervening opportunities."

TABLE 1
DISTANCES FROM LAKE VAN OBSIDIAN SOURCES AND PERCENTAGES OF OBSIDIAN ON
ARCHAEOLOGICAL SITES DATING BETWEEN 7500 AND 5500 B.C.

SITE AND LEVEL OR PHASE	% OBSIDIAN	DISTANCE (km.)
1. Çayönü, Surface to I	48.2	195
2. Çayönü, I-4	35.4	195
3. Çayönü, 4-5	17.6	195
4. Jarmo II	45.0	415
5. Jarmo I	28.1	415
6. Bouqras III	27.9	410
7. Bouqras II	25.7	410
8. Bouqras I	32.3	410
9. Tell Shemshara, 16	81	365
10. Sarab	1.7	650
11. Ali Kosh, Mohammad Jaffar	1.7	815
12. Ali Kosh, Ali Kosh	2.1	815
13. Ali Kosh, Bus Mordeh	0.9	815
14. Tepe Guran, V	2	675
15. Tepe Guran, U	0	675
16. Tepe Guran, T	46	675
17. Tepe Guran, S	6	675
18. Tepe Guran, R	0	675
19. Tepe Guran, Q	7	675
20. Tepe Guran, P	22	675
21. Matarrah, Lower Levels	8.7	450

A second major problem (also commented on by Binford) is that of trade and social stratification. There is no evidence to suggest that trade *alone* can bring about the evolution of social stratification from egalitarian or ranked societies. On the contrary, ethnographic data (e.g., Sahlins 1965) show quite clearly that "redistribution," not "accumulation," is the hallmark of trade systems below the level of what Fried (1967:185-230) has termed "Stratified Societies." It seems apparent that trade would generally militate against the evolution of a stratified society from a nonstratified society. In nonstratified societies, goods continually move in and out of the local group. Mechanisms have originated which prevent the accumulation of goods by members of an egalitarian or ranked society, such as the gaining of prestige through the redistribution of trade goods. Fried (1967:204) comments that

the trouble with trade as a cause of stratification is that, other things being constant, the flow of goods in and out of a nonstratified society will be regulated by mechanisms neutral or probably antipathetic to the formation of stratification.

In contrast, trade might further strengthen already existing social strata, as it seems to have done in Mesopotamia during the Protoliterate and Early Dynastic periods when city-states were forming. Here, such sites as Murian (flint), Tal-i-Iblis (copper), and Tilki Tepe (obsidian) lack evidence of social stratification during the Ubaid period, but had control of strategic resources. Sites such as Eridu in the alluvial valley of Mesopotamia show evidence of social stratification during the Ubaid, the importation of raw materials, and functional categories which crosscut kinship lines, such as priests. Thus, it was the *importers* of the raw materials who achieved the social stratification, not the *producers*. (Cf. Leach's [1954] instructive case of the jade trade in Highland Burma.)

A major question, then, is what was the social structure of the Early Bronze 1 society in the Aegean? If trade was a major factor in bringing about urbanism, we might predict that social stratification already existed in the region, and that long-distance trade merely strengthened and intensified it during Early Bronze 2. If these social strata did not already exist, I would be inclined to test for other factors: e.g., changes in settlement patterns leading to surplus increases, which in turn lead to stratification. The *needs* Renfrew speaks of as best being met through trade would then be reflections of new differential statuses and roles.

The above comments are in no way intended as criticism of Renfrew's

theoretical approach. With the appearance of his paper, and his earlier works, we have advanced a giant step in our understanding not only of prehistoric trade, but also of its role in culture change.

Reply

by COLIN RENFREW

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Wright's constructive comments upon the lapse rate model for the early traffic in obsidian in the Near East highlight several practical and important problems in the analysis of the distribution of this and other trade commodities.

It is useful in the first place to bear in mind that this model, like any other, is a construct which we deliberately impose upon the data to aid in their interpretation. The correlation of the percentage of obsidian in the lithic industry (expressed exponentially) with distance from the edge of the supply zone suggests that distance was, at this time, the dominant factor regulating the obsidian distribution. Naturally it was not the only such factor, and the operation of other factors will probably be reflected in deviations from a linear relationship.

As Wright points out, functional differences between sites are likely to be reflected in such deviations (as for example in the case of Sarab). Differences in geographical situation, for example the location of Khirokitia on the island of Cyprus, are evidently significant. And perhaps the interestingly low figures for Çayönü, which he quotes, may have to be explained in terms of other cultural factors. Of course the concept "supply zone" is itself a construct: to view the zone of effective supply as a precise circle centred on the source and of radius 200 km. is evidently to simplify the situation considerably. Deviations from this geometric simplicity are not surprising, therefore, but they do at least emphasise aspects of the data which may require explanation.

The addition of the Çayönü data, given by Wright in his Figure 1, is useful. But the wide variation in the percentages of obsidian from the different strata of Tepe Guran is perhaps misleading, reflecting in the first instance the very small size of the sample for each stratum. The device of plotting the distance as well as the percentage on a logarithmic scale should also be noted. A straight line in this case would indicate not an exponential decline with distance (as in the model which I proposed) but an inverse power law, with the percentage declining proportionally to the n th power of the distance (as for example in the inverse square law). Further research would be

required to distinguish more effectively between these possibilities, and in the second case to obtain a meaningful figure for the power index (which depends on the slope of the line). Meanwhile the exponential decline seems the simpler model and is, as I have shown in a recent paper (Renfrew, Dixon, and Cann 1968), open to a rather straightforward interpretation.

The supply of flint is indeed a key factor when obsidian/flint ratios are being considered, as is effectively the case here, and we shall have to devise some means of measuring and expressing the readiness with which flint was available. An alternative, as Wright suggests, would be to use instead the mean weight of obsidian fragments from each site. Unfortunately, however, the problem of different excavating and recovery techniques becomes dominant here; for while the obsidian/flint ratio is at least relatively stable to different excavating methods (for example, the presence or absence of sieving as a routine recovery procedure, and the size of mesh), the mean size or weight of samples is dependent at least as much on the recovery procedure as on the nature of the population being sampled. If one excavator uses a screen with an 8 mm. aperture, and another, one with a 3 mm. aperture, the mean weight of obsidian fragments recovered may be startlingly different. Thus some standardisation in excavation procedures will be necessary before we can follow up this suggestion.

That trade can at times operate to increase social stratification (or at least its documentation in the material record available to us) is surely demonstrated in numerous cases through mediaeval Florence and Augsburg to the present day. Doubtless, as Fried (cited by Wright) suggests, the flow of goods in and out of a society need not always result in the development of stratification. The key point in the Aegean is not, however, that the flow of goods increased in *quantity* at the beginning of the Early Bronze 2 period (although this was clearly the case), but that it increased in *variety* with the development and exploitation of many new commodities, many of them related to the growth of metallurgy. This greater range of goods certainly made possible a greater range in the material expression of social stratification. And in a display-conscious society (like the Minoan-Mycenaean civilisation, or indeed Western civilisation today) there is a very real two-way link between the expression of social stratification or social position and the stratification or position itself.

We may picture the Aegean Late Neolithic as a society with a low level of stratification (although our information on this point is decidedly inadequate).

Doubtless each village had its leaders and prominent community members, and Fried's term "nonstratified" does not seem appropriate. The problem is to explain how such a society was transformed first into the proto-urban Early Bronze Age condition and then to Minoan Mycenaean civilisation. My suggestion is that the development of metallurgy and trade made possible a number of positive feedback loops in the system. For instance the activity in or control of trade, first in a small way, by a village leader or leaders, could operate both to promote trade (through a measure of specialisation, of social emulation, etc.) and to enhance the status of the leader. Such a situation is perhaps the best explanation of the finds at Early Helladic II Lerna, where the House of Tiles and its great deposit of sealings already seems halfway towards the comprehensive economic organisation reflected in the Mycenaean palaces.

I am sure that Wright is wise in expressing caution, and the role of trade as a factor promoting or increasing social stratification could easily be exaggerated. The essential point perhaps is that we have now to examine in detail the mechanisms which can operate to produce change in culture systems. Trade may not prove in most, or even many, cases to be a dominant factor, but it is at least, through the nature of the archaeological material, one of those most open to investigation.

On a more general level, Klejn's trenchant criticisms clearly lead towards some fundamental points of difference.

It is, no doubt, always somewhat provoking to be told that one's ideas are 30 years out of date: "How funny it is to meet them again in British archaeology of the late '60's!" I can only reply that these, for me as for many colleagues, are living problems. If they have already been resolved, it would be good of Klejn not simply to say so, but to give some indication of their solution. Nonetheless, Klejn's position harmonises, in many ways, with that which I was trying to put forward. And if this admission is seen as unconsciously adopting a Marxist approach, I am emboldened by Klejn's curious suggestion that Grahame Clark does so consciously!

Accused both of monism and of pluralism, I should like to reiterate the view that trade should be seen simply as one of a number of subsystems working within the cultural system. "Trade" lies at the centre of my schematic diagram since it is the principal subject of the article. Had "craft specialisation," for instance, or "synoecism" been the subject, no doubt a convenient diagram could have been constructed, with either at the centre, to show the suggested interrelations. If we are to consider a

specific factor, such as trade, to single it out is not "monism," any more than to insist that many interrelated factors are operating is "pluralism." The answer, *pace* Klejn, is not a pangolin, but the concept of *system*, which permits one to consider subsystems without losing sight of the integral whole.

Klejn's solution to the problem, that "the need for innovation is in the nature of human beings," does not really seem "less ambiguous" than the others discussed—although I agree that the consideration of human needs is not necessarily "superficial reductionist" as Binford (CA 10:163) would hold.

The entire purpose of the approach which some of us are trying to develop is to go beyond the unitary, holistic definition of "culture" as an archaeological unit. Of course archaeologists will continue, and very properly so, to speak in terms of "cultures," but this need not prevent our seeking other configurations among the material that will help us to understand the mechanism of culture change. This concept of culture, applied automatically and unquestioningly, has become a panjandrum (if not a pangolin) of the '50's and '60's. It is, after all, no more than a construct, like many of the other concepts which we use (as Soviet archaeology in the early '70's may do well to note).

Here, then, is one fundamental difference of approach. Today it is relevant to ask for a closer definition of this term, if it is considered as having more than an operational significance. It would be helpful also if Klejn would clarify such terms as: "inherent laws of development," "inherent factors of development," and "the unity of developmental laws."

Klejn's remarks illustrate the considerable gulf which exists between archaeological thinking in Soviet Russia and that in Western Europe and America. Undoubtedly our ignorance of Soviet theoretical writings, such as those listed by him in his bibliography, must be very much to our own disadvantage. CURRENT ANTHROPOLOGY could be of service to us all by soliciting from Klejn and his colleagues a review article on the Soviet view of prehistoric archaeology today—on evolution, and on the definition and meaning of culture—focussing on theoretical problems rather than on material finds and discoveries.

Klejn's reflections are stimulating as well as provoking. A more complete and positive statement of his present position on the nature of culture and the process of culture change (and not, of course, restricted to the question of trade) would be of very real interest. I hope that he, and the Editor of CURRENT ANTHROPOLOGY, will afford us the opportunity of discussing these questions with Soviet

scholars. Perhaps the points of difference are not, after all, so numerous as has sometimes been assumed?

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On Museums and the Teaching of Anthropology

by JAMES MCW. KELLERS

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I applaud Borhegyi's efforts and am an admirer of much of his work, but his recent article (CA 10:368-70) indicates to me that he has overlooked an important avenue of communication. The exhibits he uses as examples depend entirely upon reading and the visual perception of an aesthetic experience. Thus they utilize only one of the five senses.

I am an interested museum visitor, up to a point, and when I encountered Borhegyi's statement that the average length of inspection per exhibit case was 30 seconds, I didn't believe it. On a visit to the recent exhibit of primitive art at the Metropolitan Museum of Art, I timed my inclination to move and that of other visitors, and I now think that 30 seconds may represent a long examination. Visitors carrying a taped guide stayed longer, and small groups with a personal guide stayed longer yet. The inspection span of the last type of visitor could be timed in minutes.

The exhibit was well done, but I still got tired—not because there was too much, or because the "labels" were uninformative, but because I could not touch, smell, or try to use any of the pieces on display. I became tired, not because the displays were static, but because I was forced to be static, barred from experiencing, in any way but visually, the emotions of the maker or user. Typical foot-tapping music from the area, for one thing, would have helped by alleviating the discomfort caused by the necessity to stand still, as it is a biological fact that it is usually more tiring to stand in one spot than it is to walk or move about. Other ways of increasing viewer participation in the displays might also have been found.

Several years ago I had the opportunity to produce a display attempting to attract people's attention to an industrial harbor complex. The display included a continuous slide show and narration. Experimenting, we discovered that we drew and held the largest crowd (a) when we had something for them to touch and use (rope, rope and fids, and a small ropemaking machine) and (b) when the narration was live and anyone could ask questions at any time. The message apparently got across, for improvements in harbor utilization were made.

My experience with exhibits at Boy Scout shows has further convinced me that participation is all-important, for young and older alike. The chance to try one's hand, to try to be master of a given situation, is without peer as a learning vehicle. A display of drums we arranged as part of a larger show almost literally brought down the house, for we permitted visitors to beat all but the most decrepit specimens and encouraged dancing to the beat. We also periodically demonstrated and explained how different drums were used for different emotional or musical effects. The 30-second rule set forth by Borhegyi was replaced by a 40-minute rule, and some of the older visitors found chairs and stayed for several hours.

The approach through participation can be used in almost any situation. If you want to show a ghetto apartment as a House of Horrors, you must assail *all* of the viewer's senses: Does the exhibit *smell* "right," does it *feel* right, does it *sound* right? How close is it to the original in *all* aspects?

Of course, it is difficult, if not impossible, to permit constant handling of irreplaceable museum specimens, for this amounts to sacrifice. However, reproductions, made for the purpose of handling, using, smelling, and hearing as

well as seeing, could be provided for many type-specimens. Museum staff demonstrators should do more than just demonstrate; they should invite, encourage, and lead visitor participation, even if only by story-telling and asking questions.

Borhegyi's description of a series of exhibits designed to impart information and hold the visitor's attention reflects the static nature of too many museum exhibits. His example seems to me precisely the kind of exhibit he wants to avoid—the exhibit that does not really involve the viewer.

Man is interested in man and constantly compares himself as an individual with other men, both as individuals and as a group. Does the museum give him this chance? Is the viewer, in his own mind, better or worse, different from or the same as the people represented directly or indirectly in museum exhibits? Would the viewer tackle the problem in the same manner as is depicted? What would be the results? What better way is there for man to compare, *understand*, and "find out" than by active participation?

It is my opinion that most museum displays really involve the museum staff only during the time the exhibit is planned, designed, and installed. From installation until the show is taken out, the staff is not directly involved with the display or the public. I propose that if we wish to hold our museum visitors, to get them involved, *museum staffs must become more involved*; and in particular, they must try to provide the visitor with the opportunity for participation.

[This comment was sent to Stephan de Borhegyi for a reply. He died in an automobile accident (on September 26, 1969) before he could respond. Kellers was then asked if he wished to modify his comment. He asked only to express his feeling of personal loss at Borhegyi's death.—EDITOR.]